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A Note on the Statistical Power of Regression Analysis

Poikolainen, Kari

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Response to Dr. Holmes and Dr. Angus

Kari Poikolainen*

Department of Public Health, University of Helsinki, Helsinki, Finland

*Corresponding author: Ilmattarentie 7A, 00610 Helsinki, Finland. Tel.: +358-50-4360 567; E-mail: kari.poikolainen@bastu.net

I thank Dr. Holmes and Dr. Angus for their interest in my work. Nevertheless, I have to disagree with their approach to data analysis.

1. The crucial test for the total alcohol consumption model is whether stern policies (especially higher price and more restricted availability of alcoholic beverages) can reduce alcohol-related harm or not. My indicator of harm was DALYs lost due to alcohol use. It is the most comprehensive measure of ill-health. The important question in my study (Poikolainen, 2015) is, whether stern alcohol policy can decrease DALYs lost due to alcohol use or not. Hence the reader should focus on the strong lack of association shown by the regression coefficient ($P=0.8$) and partial correlation ($r = -0.01$) between the alcohol policy index and DALYs, rather than on the non-significant partial correlation between alcohol policy and consumption ($r = -0.27$). Much should not be made of the fact that small correlations can be significant in large data sets. Alcohol consumption is only an intervening variable in the total alcohol consumption model's presumed causal chain: policy measures \rightarrow consumption \rightarrow alcohol-related harm. If total consumption model were as strong as its advocates believe, high correlations should be found in data from 30 countries. My

conclusion that the total consumption model fails is not based only on statistical inference, but on all available evidence.

2. As explained in both my studies, numbers of cases were studied instead of rates, since numbers divided by a common variable (such as population size) can produce spurious results in correlational and regression analysis (Kronmal, 1993). Although commonly overlooked, this has been a statistical fact over 100 years.

Multicollinearity does not reduce the predictive power or reliability of the regression model as a whole. If notable, it may influence the precision of estimates for individual independent variables. Multicollinearity is not a problem in the later study (Poikolainen 2016). Partial correlation (controlled for population size, as usual) between the independent variables, abstainers and alcohol dependents, is low, -0.13 ($n=28$; Iceland and Turkey excluded, reasons explained in the study). Farrar-Glauber test is not universally accepted (Cassidy, 1981). Variance inflation factor (VIF) and tolerance for alcohol dependents were 5.59 and 0.18. The respective figures for abstainers were 13.48 and 0.07. The little multicollinearity there is just means that the abstainer variable P -value is slightly higher and confidence interval for the regression coefficient wider than without any multicollinearity. Still, the association was highly significant.

3. The highest percentage difference (+356%) between the recorded and estimated per capita alcohol consumption pertains to Luxembourg. It is noted in the study that this is a tiny country with a lot of visitors and cross-border shopping. Since recorded alcohol consumption is based on the sales of beverages to all shoppers while number of alcohol dependents and that of abstainers are based on the resident population, it is not a surprise that there is a large difference. After excluding Luxembourg from the data set a repeat analysis showed no material change in the results. I did not report this because the PRESS statistic indicated that the model was robust and not overfitted even if Luxembourg was included.

4. Cross-national comparisons are needed to study the effects of population-level policies, since policy laws and other measures are the same for all members of any the given country and are applied to all drinkers. You cannot randomize nations or people to study policy effects. My two studies were based on data from the most developed OECD countries. Admittedly a small number of countries but the best available ones. I cannot imagine more reliable sources of national data. The question is then: Is it better to cast out all evidence or to take into consideration the evidence that is available? I rather believe what the recent data tells than remain ignorant or believe in the gimmick based on cross-sectional, time-trend, survey and alcoholism treatment outcome data (Bruun *et al.*, 1975) that was used to construct the total alcohol consumption hypothesis. To decrease harm we should focus on the problem of alcohol dependence. This is important.

Conflict of interest

Nothing related to the research and response at hand. All has been done by me during retirement. I am affiliated as unpaid adjunct professor at the Department of Public Health, University of Helsinki, Finland. Other: Fee received for proof reading a report. Panimo- ja virvoitusjuomateollisuusliitto ry (The Federation of the Brewing and Soft Drinks Industry, Finland).

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Letter to the Editor

A Note On The The Statistical Power Of Regression Analysis

Kari Poikolainen*

Department of Public Health, University of Helsinki, Helsinki, Finland

*Corresponding author: Ilmattarentie 7A, 00610 Helsinki, Finland. Tel.: +358-50-4360 567; E-mail: kari.poikolainen@bastu.net

I reported a non-significant association between an index of stern alcohol policy efforts and disability-adjusted life years (DALYs) lost due to alcohol use in this journal, suggesting that the total consumption model fails (Poikolainen, 2016). Dr. Holmes and Dr. Angus (2017) thought that "This is a classic case of misinterpreting non-significance as evidence that the hypothesis is false rather than as a lack of evidence that the hypothesis is true." They did not present any analysis of statistical power (1 - beta error). Had they done it, the calculations would have shown that the statistical power is high in my regression analysis even if there were only 30 countries, because R^2 was high and the number of regressors limited. Anybody can now check this because a useful calculator for the beta-error has been made freely available (Soper, 2017). References to the statistical literature have been posted on the website. Thus, my results constitute evidence of absence, not absence of evidence.

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